The Institution is not, as a body, responsible for the opinions expressed by individual authors or speakers

THE PROCEEDINGS OF
THE INSTITUTION OF ELECTRICAL ENGINEERS

EDITED UNDER THE SUPERINTENDENCE OF W. K. BRASHER, M.A., M I.E.E., SECRETARY

VOL. 97. PART I (GENERAL), NO. 106. JULY 1950

621.3 : 538.1 MEASUREMENTS SECTION

SOME ELECTROMAGNETIC PROBLEMS

By Professor G. W. O. HOWE, D.Sc., LL.D., Member.

(*The Lecture was delivered before the* MEASUREMENTS SECTION *3rd May, the* SCOTTISH CENTRE *at* GLASGOW *22nd November, the* NORTH-WESTERN CENTRE *6th December, and the* NORTH LANCASHIRE SUB-CENTRE *7th December*, 1949; *and before the* NORTH-EASTERN RADIO AND MEASUREMENTS GROUP *16th January, the* SCOTTISH CENTRE *at* EDINBURGH *1st March, the* RUGBY SUB-CENTRE *14th March, and the* SOUTH MIDLAND RADIO GROUP *at* MALVERN *15th March, and before a Joint Meeting of the* SOUTH MIDLAND RADIO GROUP *and the* SOUTH MIDLAND CENTRE *at* BIRMINGHAM *1st May*, 1950.)

It is now 46 years since I first had to explain the principles of electromagnetism to a class of students, and during the intervening years I have had many discussions on problems that are continually cropping up, and have written many articles explaining these problems, and combating what I considered to be heretical views.

When I was asked to give this lecture I had to decide on a subject, and, after much consideration and discussion with friends, I decided to review a selection from these various problems. They are all such as must interest anyone in any way concerned with the fundamental principles of electrical measurement.

As I said in the opening paragraph of an article in *The Electrician*[1] in 1915, every student of electrical engineering who makes a serious attempt to get clear physical conceptions of the fundamental principles of electromagnetic induction is faced sooner or later with a problem which presents itself in various forms, but which is, perhaps, most often expressed in the question: If a cylindrical bar magnet or a solenoid be rotated about its axis, will its magnetic field rotate with it? I was under the impression when I published that article in 1915 that I had satisfactorily disposed of this question, and that it would never arise again. How mistaken I was became very evident 20 years later.

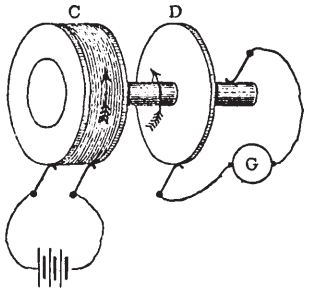

Fig. 1

If, in Fig. 1, C is a stationary coil carrying a steady current and D a disc rotating about the same axis, a current certainly flows in the galvanometer G. If now the disc is stationary and the coil rotating, sliding contacts being used to carry the current into and out of the coil, there is no reading on the galvanometer. This cannot be regarded, however, as a proof that the field does not rotate with the coil, for a believer in the rotating field would maintain that it induced equal and opposite e.m.f.'s in the disc and connecting wires. One might say that it should surely be possible to give a definite answer to such a simple question, but this is not so because the question is meaningless. Whether the coil be rotating or not, the disc is situated in a magnetic field which is undergoing no change whatever either in strength or direction. The lines of force are mathematical abstractions, and it is merely a convention whether one pictures a hundred or a million of them per unit area. One is not justified in regarding them as physical entities like bristles that one can chalk-mark and thus trace their movement. If the magnetic field of the coil is unchanging in magnitude and direction when the coil rotates, it is therefore meaningless to ask whether the field is rotating or not, and it is not surprising that all the experiments that have been made to decide the question have proved quite inconclusive. It would be surprising were it otherwise.

An interesting problem is the mechanical reaction which must oppose the driving torque when current is taken from the rotating disc. In an ordinary dynamo the torque is exerted by the tangential component of the magnetic field acting on the fixed poles, but in the case of the coil and disc one cannot imagine a distribution of the field capable of exerting a torque on the coil. In this case the torque is exerted on the external circuit, which will tend to rotate with the disc. This can be seen at once by making the external circuit a radial wire or a fixed disc close to the rotating disc (Fig. 2). If the direction of the current is outwards in the rotating disc, it will be inwards in the external circuit, and the two torques must be equal and opposite. When I first discussed this problem in 1915, my statement that a line of force is skewed in passing between the discs and consequently does not come back to the point from which it started met with some opposition. One must, however, banish the bristle theory, according to which every line of force is a physical entity in the form of a closed loop. Starting at any point and tracing out the line of force, the skewing that takes place between the discs

Until his recent retirement, Prof. Howe was at the University of Glasgow.

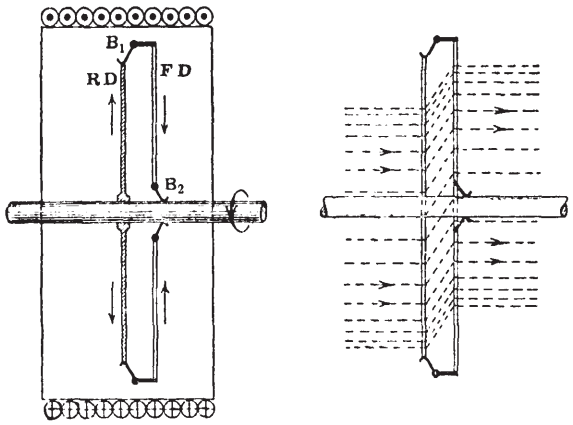

Fig. 2

causes a gradual working around the axis, but one might have to go round an infinite number of times before he happened to arrive at the exact point from which he started, and thus form a closed curve.

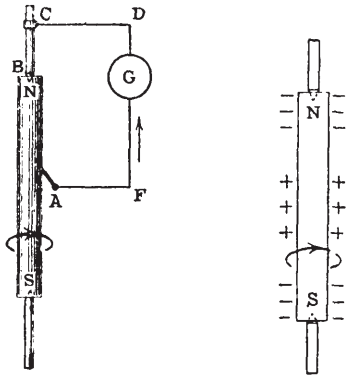

Fig. 3

If a bar magnet be rotated about its axis as shown in Fig. 3 a current will flow through the galvanometer. It has been suggested that this may be due to the rotating magnetic field cutting the external circuit, but this is difficult to conceive, since the external circuit is at rest in a magnetic field that is undergoing no change whatever, either in magnitude or direction. Inside the magnet, however, we have a conductor, namely the steel magnet itself, rotating in a magnetic field, and thus inducing an e.m.f. If the magnet be at rest and the external circuit rotated in the opposite direction, the current will be the same, but due now to the movement of the external circuit through the magnetic field.

It may be objected that since the relative motion of the magnet and wire is the same in the two cases, it is inconsistent to regard the e.m.f. as being induced in the magnet in one case and in the wire in the other. This raises an important point, namely the absolute character of rotation. When we say that a body is moving, we mean relatively to some other body, but the rotation of a body is not relative, it is absolute; a body is either rotating or it is not, and in the above example the e.m.f. is induced in the body which is rotating.

If the bar magnet be rotated without any electric circuit, the electric forces are induced in it as before, and, as it is brought up to speed, minute currents flow and cause a distribution of electric charge, not only on the surface as shown in Fig. 3—it is not an electrostatic condition—but throughout the material. The distribution adjusts itself to give at every point an electric force exactly equal and opposite to that induced by the rotation. The surface charges produce an electric field in the neighbourhood, and if a stationary metal disc were situated axially near one of the poles, this electric field would cause a distribution of charge in the disc, exactly the same as would be produced in it if the magnet were at rest and the disc rotating in its magnetic field.

An interesting problem, which was discussed by Poincaré in 1900,[2] is that of an ebonite annular ring rotated between two fixed metal rings with the pole of a bar magnet at the centre (Fig. 4). The metal rings are connected by an external wire and

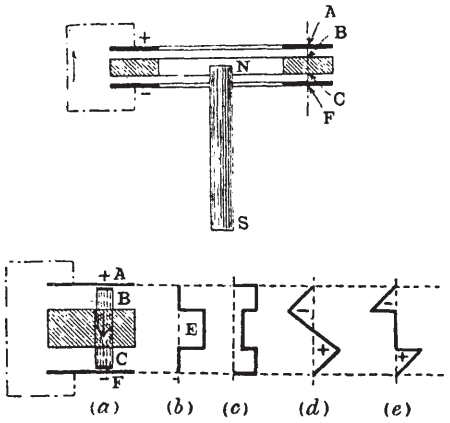

Fig. 4

constitute a condenser with a rotating dielectric. As the speed is increased a current flows in the wire and the condenser is charged owing to the electric force induced in the dielectric by its rotation in the radial magnetic field. Although the condenser is charged the metal plates are at the same potential. (*a*) shows the electric displacement D in the air-gaps and ebonite, (*b*) the electric force induced in the latter, due to its rotation in the magnetic field, (*c*) the resultant electric force distribution which must be $4\pi D$ in the air-gaps and $4\pi D/\kappa$ in the ebonite. If the distance between the metal plates be d, and the thickness of the ebonite ring t, then $4\pi D(d - t) + 4\pi Dt/\kappa = Et(1 - 1/\kappa)$ where $E = Bv/(3 \times 10^{10})$. Poincaré stated that the potential undergoes a sudden jump of $Et/2$ at the surface of the ebonite, as shown at (*e*), but in this he was certainly mistaken, the actual distribution of potential being as shown in (*d*). We return later to the subject of the e.m.f. induced in the ebonite and the factor $(1 - 1/\kappa)$.

Up to this point I have confined myself to matters which I discussed in 1915. In 1933, having heard that the rotation or non-rotation of the field of a bar magnet was being discussed, and even suggested as a suitable subject for post-graduate research, I devoted an editorial article[3] to a restatement of the meaninglessness of the question. I pointed out that if a lamp with a perfectly uniform distribution of candle-power were rotated above a table, one could ask if the illumination of the table would rotate, or, if a uniformly heated disc were placed on the table and radial lines of heat flow or temperature gradient plotted on the table, one could ask if these lines would rotate when the disc was rotated. If the source of light or heat or magnetism be not uniform about the axis of rotation, the result of rotation might be regarded by a superficial observer as an obvious proof of the rotation of the illumination or temperature or magnetic induction, but a closer consideration will show that these conditions are merely undergoing such changes at every point that their inequalities of distribution rotate. The illumina-

tion or temperature or induction at A may now be equal to what it was a moment ago at B, but it does not follow that any physical reality which now exists at A is actually that which was at B and that it must necessarily have rotated; in fact, such a statement appears meaningless.

In the same editorial I referred to an interesting experiment described by Blondel,[4] who placed a drum of insulating material between the poles of a powerful electromagnet and rotated it rapidly so as to wind an increasing number of turns of insulated wire on the drum, the wire being unwound from another drum outside the magnetic field. By means of brushes rubbing on a slip-ring on each drum, the "circuit" was completed through a galvanometer, which, however, gave practically no indication of a current, although the linkages of the "circuit" were being increased. The "circuit" here was fictitious; it consisted in part of a continually changing and arbitrary dotted line drawn on a slip-ring, which made it impossible to apply the conception of linkages. At no point was the magnetic field undergoing any change, and no part of the circuit was moving through the magnetic field in such a direction that it cut the lines of magnetic induction.

In some cases, such as transformers, there are no moving conductors and one need only consider variations of the magnetic flux; in other cases such as d.c. generators there are no variations of the magnetic field and one need only consider the movement of the conductors. In cases in which there are both field changes and conductor movements the explanation is not so simple, although it can often be simplified by describing the phenomena from the point of view of an observer on the conductor or on a magnetic pole.

When a conductor and a magnetic pole are moving relatively to one another as shown in Fig. 5(*a*), to an observer on the pole

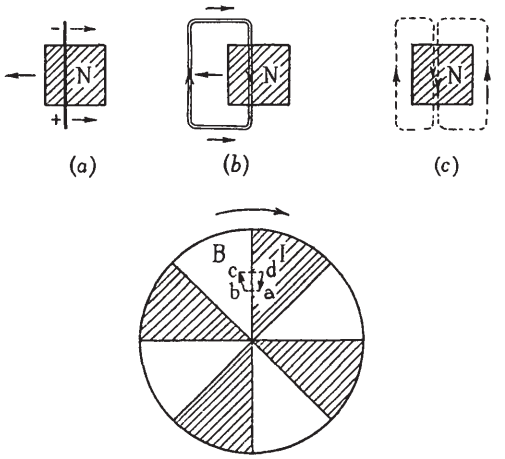

Fig. 5

the e.m.f. induced in the conductor is due entirely to its movement in the unchanging field. To an observer on the conductor the pole is moving and the explanation would be different, unless he had a flash of genius and saw that it was preferable to assume that the pole was at rest and that he himself was moving. Owing to the movement of the pole the magnetic fluxes through the closed dotted curves in Fig. 5(*c*) are changing and inducing e.m.f.'s around the paths. At first sight one might think that this would produce electric forces in the pole as well as in the conductor, but the pole is moving through its own magnetic field and thus experiencing an equal and opposite electric force.

An interesting problem that I considered in July, 1939,[5] is the rotation in an axial magnetic field of a long cylinder made up of alternate sectors of brass and iron (Fig. 5). In such cases one is tempted to endow the lines of force with physical reality and picture them going round with the cylinder like sheaves of bristles cutting the stationary external circuit. If we do not do this, however, but approach the problem fundamentally, then we have the brass sectors rotating in a flux density B and the iron sectors in a flux density μB, and yet, if the flux does not rotate, the total flux cut by any radius in a complete revolution must be the same. When the flux density at any point is undergoing changes, the induced e.m.f. due to these changes must be taken into account. Consider the dotted path *abcd* of 1 cm radial depth to be stationary in space; then, at the moment shown, the junction between brass and iron is sweeping across the Figure with velocity v, brass replacing iron, and the flux consequently decreasing, and, assuming the direction of the flux to be away from us, inducing an e.m.f. of $(\mu - 1)Bv10^{-8}$ volt around the path in the direction shown. A half of this will act along *bc* and be added to the e.m.f. due to the rotation, giving a total in the brass of $[Bv + \frac{1}{2}(\mu - 1)Bv]10^{-8}$ volt, while the other half will act along *da* and be subtracted from the e.m.f. due to rotation, giving a resultant in the iron of $[\mu Bv - \frac{1}{2}(\mu - 1)Bv]10^{-8}$ volt. Both of these are, of course, equal to $\frac{1}{2}(\mu + 1)Bv \times 10^{-8}$ volt, which is the e.m.f. induced in 1 cm of any radius whether in the iron or the brass. If the angular velocity is ω and r the radius at the point considered, this is equal to $\frac{1}{2}(\mu + 1)B\omega r \times 10^{-8}$, and on integrating this from 0 to R, where R is the radius of the cylinder, we obtain $\frac{1}{2}(\mu + 1)B\pi R^2 n \times 10^{-8}$ volt, where n is the angular velocity in revolutions per second. This could, of course, be written down at once from a consideration of the rotating radius.

In the absence of any external circuit a positive charge accumulates on the external surface and an equal negative charge is distributed uniformly throughout the material with such a density that its field exactly counteracts at every point the electric force due to the rotation; both are proportional to the radius.

At the British Association meeting in 1933 the late Professor Cramp[6] read a paper entitled "Axial Spin of Magnetic Field," and at the meeting in the following year I read a paper[7] entitled "A Perennial Chimera," in which I restated briefly the arguments of my earlier publications. Under the guidance of Professor Cramp a number of experiments had been made at Birmingham University with the object of finding an answer to what I had maintained to be a meaningless question. In discussing some of Faraday's work, Professor Cramp said, "Curiously enough Faraday's description of his experiments is for once lacking in detail. He gives no hint whatever as to the disposition of his galvanometer leads, so that *he has omitted the possibility* of the e.m.f.'s being produced by a rotating flux cutting the stationary parts of his circuit." With reference to my 1915 paper he said that the case was considered "*but without experimental confirmation.*" As I said at the time, it was gratifying to have committed an alleged sin of omission in such illustrious company, but Faraday could hardly be blamed for not foreseeing the misconceptions which would subsequently arise through a too materialistic adoption of his lines of force, nor could I be blamed for not seeking an experimental answer to a question which I maintained was meaningless. Professor Cramp himself supplied the most telling rejoinder to his criticism when he stated, after making about 50 experiments, that "as experiments they were not conclusive."

A strange repercussion from the discussion with Professor Cramp was a letter in *The Electrician* in 1938 from Professor Cullwick[8] in which he said, "Are not electric and magnetic fields merely hypotheses which have been illogically endowed with reality? . . . As to the chief issue . . . I find that I am definitely in agreement with the late Professor Cramp on one point; that

his problem has meaning. I do not believe that he has been chasing a chimera or tilting at windmills. . . . With his conclusion that the field cannot be regarded as rotating, however, I am sorry to say that I find I cannot agree." To this I replied,[9] "How experiments made to determine the rotation or non-rotation of a hypothesis illogically endowed with reality can be anything but chimera-chasing passes my comprehension." To this Professor Cullwick replied,[10] "We may easily define the motion of a field as the motion of its source, and the meaning of the motion is then completely contained in our definition." Having thus defined the motion of a field as the motion of its source, what should one say of a person who then made experiments to determine whether the field rotated with the source or not?

In the *Journal* of The Institution in 1936 Professor Cramp and Dr. Norgrove[11] published a detailed description of their 1933 experiments, which I discussed in the *Journal*[12] and also in two articles[13, 14] in *The Electrician* entitled "A Persistent Fallacy" and "Magnets and Fields of Force." Professor Cramp gave an interesting quotation from Faraday's Diary, which had recently been published, showing beyond doubt that in the privacy of his diary, if not in any published paper, he had written "Do the lines of force revolve with the magnet or do they not?"

Some years before I took any interest in the subject, namely in 1900, Poincaré had written,[15] "Ces lignes de force ne sont q'une création de notre esprit. . . . La question ne peut être resolvé, parcequ'elle n'a pas de sens. . . . On n'a donc aucun moyen de décider entre les deux hypothèses." In 1912 S. J. Barnett[16] wrote, "It was first shown by S. T. Preston[17] in 1885 that the experimental results could, as is now well known, be explained equally well on either hypothesis." In *The Electrician* of the 20th November, 1936,[14] I gave a number of other references.

I should like to add here that Professor Cramp and I were great friends and used to visit each other and fight over these problems. His untimely death was a great personal loss to me.

I turn now to an entirely different problem. In the *Electrical Review*[18] of the 28th December, 1934, an interesting article by Dr. E. Hughes entitled, "The Torque of an Electrical Machine" led to some correspondence and further articles. In *The Electrician*[19] of the 7th June, 1935, I showed that the transmission of the torque from the armature to the poles of a dynamo was very different from what one usually pictured. In the absence of any compensating winding in the pole-face, whatever the tangential value of B might be in the iron, if one assume a permeability of say 1 000, the tangential value of H in the iron and therefore also in the air must be negligible; that is, the flux must enter normally into the pole with practically no tangential component. With the ordinary type of machine the torque is exerted on the sides of the armature slots and on the sides of the poles. If the pole-face has a compensating winding in open slots, the torque is exerted on the sides of these slots, but if the compensating winding is in tunnels in the pole-face, conditions are quite different, for the line integral of H around the compensating winding shows that there will now be a tangential component of H on both sides of the pole-face surface, and that the flux must enter the pole-face at an angle. These somewhat surprising conclusions were discussed more fully in a letter[20] and a further article[21] in *The Electrician* entitled, "Some Magnetic Misconceptions." In the opening paragraph of the latter I stated that "there are many magnetic phenomena about which most electrical engineers hold hazy if not erroneous views." As an example, I considered the attractive force between an electromagnet and its keeper, which is often proved in textbooks to be $B^2/8\pi$ dynes per square centimetre. If the iron keeper is replaced by a wooden one, B is reduced but can still be measured, but the attractive force is zero. When flux passes from air into iron, the Maxwellian tension in space is reduced from $B^2/8\pi$ to $B^2/8\pi\mu$ and the mechanical force is the difference $B^2(1 - 1/\mu)8\pi$, and this vanishes when $\mu = 1$. For most practical purposes the $1/\mu$ is negligible, but I used this example as an introduction to a consideration of the phenomena involved when magnetic flux passes from one medium to another. When flux enters the pole at an angle as shown in Fig. 6 it is refracted, and on calculating

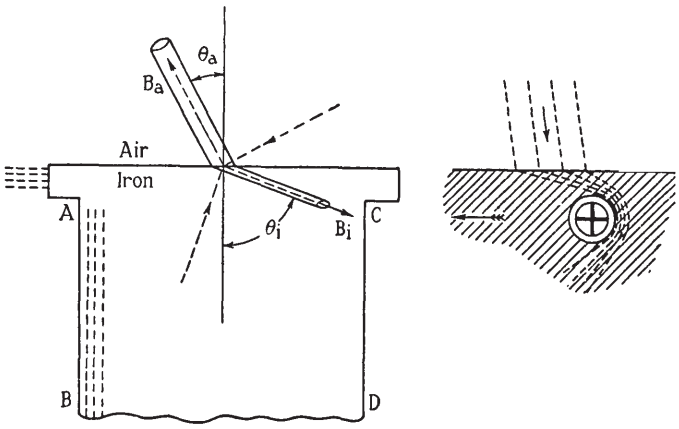

Fig. 6

the tangential stress in the air and in the iron we find that they are equal; in other words, the tangential stress of the magnetic field passes through the pole-face without exerting any mechanical force on it. Hence, even with compensating windings in tunnels, although the flux enters the pole at an angle, the torque is not exerted there but on the walls of the tunnels in a sort of catapult action due to the Maxwellian transverse pressure. Searle[22] had pointed this out in 1898. With regard to the Maxwellian stresses in space, it should be pointed out that Clerk Maxwell himself showed that they could also be regarded as a tension of $B^2/4\pi$ along the lines, combined with a hydrostatic pressure of $B^2/8\pi$ *in every direction*. As I said in 1935, perhaps the Germans are wise in referring to the Maxwellian stresses as *fiktive Spannungen* [fictitious stresses].

When magnetic flux passes into a medium of permeability μ, the division of the total stress $B^2/8\pi$ into two components, one mechanical and the other non-mechanical, has been the subject of much discussion. Helmholtz disagreed with Maxwell's solution, and Larmor by different methods obtained several "discrepant" values; I devoted the editorial[23] of December, 1946, to the subject. I pass now to another problem.

In the editorial[24] of June, 1942, entitled, "More Magnetic Fallacies," I considered some interesting problems which had been propounded by H. Stafford Hatfield in *Nature*,[25] as examples showing that "the field picture as usually stated *seems* to be inadequate." As I said at the time, when a person tells you that the room *seems* to go round, you may be sure that there is something wrong—but not necessarily with the room. In his first example he stated that "if a horseshoe magnet be rotated when placed so as to send flux through a few turns only of a short-circuited toroid, the field theory *seems* to call for an alternating current, with no reactive torque on the magnet." On the contrary, the field theory definitely rules out the possibility of such a thing. The line integral of H around any closed path in the magnetic field is zero (Fig. 7*a*), and since the cross-section of the toroid is constant, the line integral of the flux Φ is zero; and, since the winding is assumed to be uniform, it is immaterial whether one multiplies the flux by the number of centimetres or by the number of turns; hence the linkages must be zero.

In his second example he said that "if a toroid solenoid be

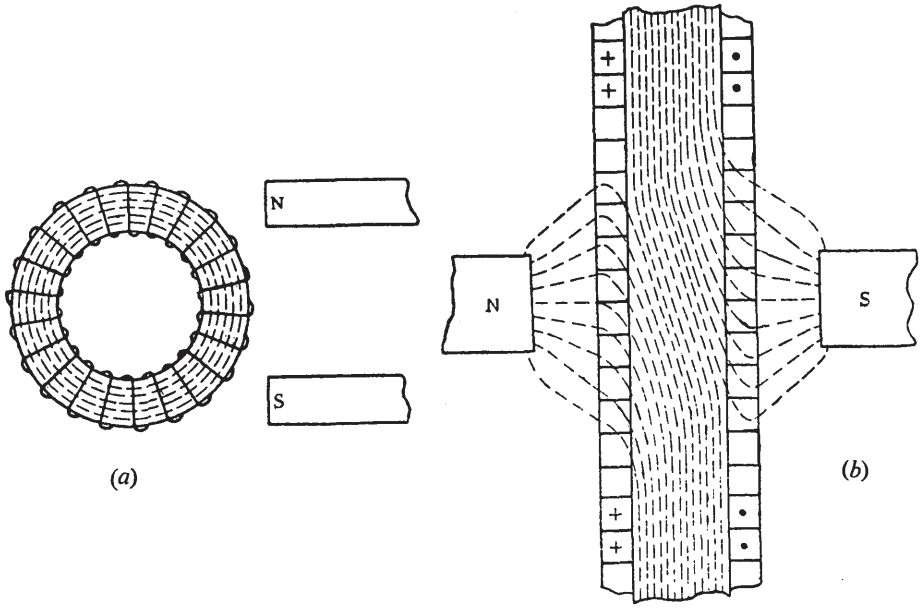

Fig. 7

placed with its plane at right angles to that of a horseshoe magnet, with a few of its turns in the pole-gap of the magnet (Fig. 7*b*), the field picture *seems* to call for a torque on the toroid but none on the magnet." From what I have just said, however, it is obvious that, since there is no linkage, there can be no torque. If current be passed through the solenoid it produces no external field and therefore cannot modify the field of the magnet outside the solenoid or exert any torque on the magnet. But surely, one might argue, the conductors carry current in one direction on one side and in the opposite direction on the other side in the transverse field of the magnet, and the forces on the two sides are such as to exert a torque tending to tilt the toroid. This is quite true, but yet the toroid does not tilt and the magnet experiences no torque. The field of the magnet enters the toroid symmetrically, but inside the toroid it is compounded with the solenoid field, which is therefore distorted so as to transmit the mechanical force internally from one side of the toroid to the other via the skewed field. The toroid shows no more tendency to rotate than does a person who tries to turn himself round by pulling at his own braces.

Stafford Hatfield, evidently an electron enthusiast, said that "historical piety should not lead us to burden the beginner's mind with the difficulties and artificialities of the magnetic field picture," but, as I said at the time, it requires no historical piety to admire the beauty of this magnetic field picture.

Another point raised in the same article was the question whether a piece of wire has a bigger inductance when bent into a circle than when it is straight. The author said that a dozen highly skilled physicists and radio research men had all given the wrong answer, but I put this down to their having been brought up on an electron diet, and probably suffering from a magnetic field deficiency. The question is really meaningless, because one cannot have a steady current flowing in a straight piece of wire isolated from all other current-carrying conductors. If one overlooks this difficulty and applies the ordinary Biot–Savart formula, one finds on integrating throughout the surrounding space that the result is infinite. If one adopts the arbitrary procedure of imagining two planes normal to the conductor through its ends, and neglects all the magnetic flux outside these planes, one obtains the result often given in tables of formulae. A similar procedure is adopted in obtaining the formula for the mutual inductance between two parallel bits of wire. The difficulties here are not due to any magnetic field picture but to the attempt to define something which is physically impossible and inconceivable.

Turning now to an entirely different problem, let us consider a cylinder of brass rotated about its axis in a magnetic field parallel to the axis, with brushes rubbing on the external and internal surfaces and connected to a voltmeter. If Φ is the magnetic flux through the cylinder and n its speed of rotation in revolutions per second, the p.d. between the brushes will be $n\Phi \times 10^{-8}$ volt. If now the brass cylinder be replaced by one of ebonite with thin brass inner and outer linings, what will be the p.d. as read on an electrostatic voltmeter? Lorentz and Larmor disagreed with Hertz on this point, and elaborate tests were made in 1905 by H. A. Wilson[26, 27] in the Cavendish Laboratory to decide the question; the tests confirmed the views held by Lorentz and Larmor. The tests were made at 12 000 r.p.m. in a flux density of 200 gauss produced by a water-cooled solenoid, and the p.d. was measured on a quadrant electrometer. In traversing a radial path through the dielectric one would pass through a number of atoms each with electrons capable of elastic displacement when subjected to electric force, and between the atoms empty spaces. This raises two questions: Is one justified in taking the whole radius or only a part of it in calculating the induced e.m.f.; and will all the induced e.m.f. or only a part of it be read on the voltmeter? The answer in both cases was found to be—only a part. The measured voltage was exactly what would be obtained if the ebonite cylinder were replaced by a number of concentric brass cylinders with air spaces between them, arranged to have the same capacitance. The fraction of the radius occupied by brass would be $(1 - 1/\kappa)$, and the induced e.m.f. would be reduced in this ratio. The air spaces constitute an internal condenser of capacitance C in series with the quadrant electrometer and stray capacitances amounting, say, to C'. The observed voltage would therefore be $V = EC/(C + C')$ where $E = n\Phi(1 - 1/\kappa) \times 10^{-8}$.

Eight years later Wilson[5-28] repeated the experiment but with a magnetic dielectric. Since such a material was not known at that time he made one by coating a number of $\frac{1}{8}$-in steel balls with sealing wax, putting them in a brass hollow cylinder, and then filling it with molten wax. As I pointed out in 1939, since

the electric field is purely radial and the magnetic field purely axial, a compound cylinder could be made up as before, but with air-spaced iron cylinders instead of brass ones, or, in order to get a certain value of resultant axial permeability, with some cylinders of brass and some of iron. This arrangement is much more amenable to calculation than the mass of insulated steel balls and leads to the same result, namely that the induced e.m.f. is $n\Phi(1 - 1/\mu\kappa)10^{-8}$ volt, as Wilson confirmed experimentally.

In the *Philosophical Magazine* of January, 1945, there was a paper[29] on a paradox in electrodynamics which I dealt with in the editorial[30] of October, 1945. If *ids* and *i'ds'* (Fig. 8) represent

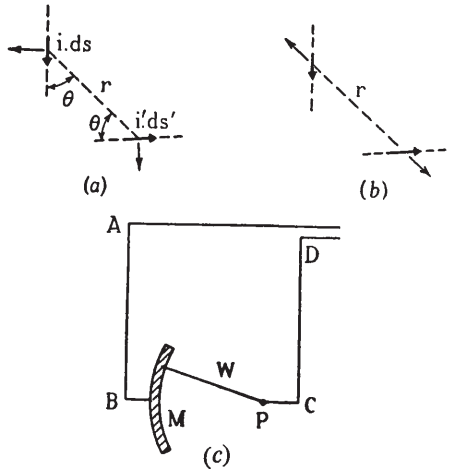

Fig. 8

two current elements in the same plane separated by a distance r and making angles θ and θ' with the line joining them, then by the Biot–Savart formula *ids* produces at the other element a magnetic field $ids \sin\theta/r^2$ normal to the plane containing the current elements, and the force on $i'ds'$ will be $i'ds' \times ids \sin\theta/r^2$ in the direction shown. Similarly the force on *ids* will be $ids \times i'ds' \sin\theta'/r^2$ in the direction shown. Hence the forces are neither equal nor in the same direction, in apparent violation of Newton's third law, but seeing that isolated current elements are contrary to Nature, they cannot be expected to conform to natural laws. In 1823 Ampère published a paper on the subject criticizing Biot's law and giving an alternative, more complicated formula which gave equal and opposite forces on the two current elements as shown in Fig. 8(*b*); it also gave a force when the two current elements are in the same straight line, and he devised an experiment to prove the existence of such a force, but it was, of course, fallacious. The real test can only be applied to the integration around the whole circuit, and it is then found that the result of either formula agrees with experiment and with Newton's laws. With the Ampère formula all the component forces except those normal to the current element cancel out.

A celebrated experiment was described by Carl Hering[31] in 1923. In the circuit shown [Fig. 8(*c*)] the arm W is pivoted at P and the other end dips in the mercury trough M. For this position every physicist that Hering consulted said that the arm would move outwards so as to increase the flux through the circuit, but as a matter of fact it moves inwards for the very same reason. Some of the explanations that have been published are very strange. The author of the paper in the *Philosophical Magazine* applies Ampère's supposed end-on force between current elements to the mercury in the trough, but the following is much more amusing: "Imagine billions of electrons rushing along from the negative terminal of the battery along the connecting wire and plunging into the liquid of the trough. Then remember that each of these electrons is about $\frac{1}{1845}$ of the mass of the hydrogen atom. Remember that for the currents used there would be about a thousand billion billion of these electrons taking that plunge each second. Now I simply raise the question, is it inconceivable that, as they do plunge, they would kick back on the slider? I do not believe it is inconceivable, and I cannot see that flux and other Maxwellian concepts enter into the problem at all." As I said in 1945, this was not written by Walt Disney but by John Mills of the Bell Telephone Laboratories. Unfortunately for this beautiful picture, a proper application of the Biot–Savart formula shows that the arm is in equilibrium somewhere about the position shown; nearer the middle of the trough the resultant force on the arm is outwards, but nearer the end of the trough it is inwards. In the editorial I gave a detailed calculation. Any apparent paradox is due to misconceptions.

The experiment can be explained simply as follows: the magnetic field due to the side BA, for example, falls off rapidly as one moves from A towards D, and the force acting on an element of AD falls off in the same way, so that the forces tending to make the rectangle expand are much greater at the corners than elsewhere. When the arm W is near the centre of the trough the outward force on it is small because it is at some distance from the other sides of the rectangle. In the position shown, however, the "corner effect" operates on the part of W close to the mercury trough, and thus, although most of the arm is subjected to a weak outward force, the part near the trough is in a reverse magnetic field due to the current in the mercury and experiences a strong inward force. A detailed calculation is given in the *Wireless Engineer* of October, 1945.

I turn in conclusion to an entirely different subject. Speculations based on classical electromagnetism are not without their interest even in subjects where modern theoretical physics has apparently introduced essentially new concepts. In February, 1946, I devoted the editorial[32] to a consideration of the size of an electron and the nature of its mass. In March, 1944,[33] I had shown that whenever a charge moves it is surrounded by a toroidal distribution of displacement currents, and the magnetic field produced by these currents was shown to be the same as that calculated on the assumption that the moving charge can be replaced by an element of wire carrying a current. If A and B are two successive positions of a moving positive charge (Fig. 9), the electric field at a point P has changed from P*a* to B*b* and *ab* represents the change in the electric field, which produces a displacement current in the same direction, the magnitude of which is easily calculated. If this is done for various points

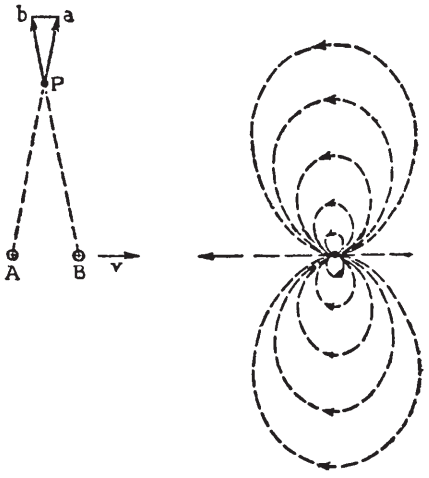

Fig. 9

around the moving charge and the paths of the displacement currents plotted, the toroidal distribution shown in the Figure is obtained; the moving charge is here assumed to be a negative electron. The magnetic field produced by these displacement currents represents an amount of energy which must have been expended in accelerating the electron; this energy is proportional to the square of the velocity and may be regarded as endowing the electron with a certain equivalent mass. We know that the charge of an electron is $4{\cdot}803 \times 10^{-10}$ e.s.u., and if we knew its radius we could calculate the magnetic energy for a given velocity and thus determine its equivalent mass. It occurred to me to reverse the process, and, on the assumption that its known mass, $9{\cdot}11 \times 10^{-28}$ gramme, is due entirely to its magnetic field, to calculate its radius. If one assumes that the electron is a sphere with its charge on its surface, one merely has to integrate its magnetic field energy from r to infinity, but if one assumes that its charge is uniformly distributed throughout its volume, there is an additional internal magnetic field to be integrated from 0 to r. On the first assumption the calculated radius is $1{\cdot}88 \times 10^{-13}$ cm and on the second, $2{\cdot}256 \times 10^{-13}$ cm. In the latter case one-sixth of the total energy is within the electron and 0·6 of the total within a sphere of twice the electron radius.

This magnetic field endows the electron with all the kinetic properties that one usually associates with mass, such as momentum, kinetic energy and centrifugal force. In considering the rotation of an electron around an atomic nucleus we can apply the usual laws on the assumption that it has ordinary gravitational mass, even if it has no such mass. If it could be proved that an electron had a larger radius than this calculated value, then its magnetic field could not account for all its mass.

This aspect of the mass of an electron throws light on something that would otherwise be very difficult to conceive. When an electron leaves a cathode the velocity with which it reaches the anode can be calculated from the anode voltage, but if this voltage be increased indefinitely, the velocity of the electron, as calculated by the ordinary formula, would reach and exceed that of light. This is ruled out, however, by the assumption that as the velocity approaches that of light, the effective mass of the electron increases. If the mass were entirely of a gravitational nature it is difficult to see how it could be increased, but if it be entirely or even partially due to its magnetic field, the matter is quite simple, since the apparent mass is inversely proportional to the radius. All the electron has to do as its velocity approaches that of light is to shrink; if its radius be reduced to a quarter, its effective mass and kinetic energy will be quadrupled without any increase of velocity.

Fig. 9 is drawn on the assumption that the electric field has the same distribution when the charge is moving as when it is at rest. At very high velocities this will be appreciably incorrect; the field will be distorted, and the results will be somewhat modified.

REFERENCES

(1) *Electrician*, "Some Problems of Electromagnetic Induction," 1915, **76**, p. 169.
(2) *L'Éclairage Électrique*, 1900, **23**, p. 41.
(3) *Wireless Engineer*, "Electromagnetic Induction," 1933, **10**, p. 409.
(4) *Comptes Rendus*, 1914, **159**, p. 674.
(5) *Wireless Engineer*, "Another Classic Experiment," 1939, **16**, p. 327.
(6) *Electrician*, "Axial Spin of Magnetic Field," 1933, **111**, p. 471.
(7) *Ibid.*, "A Perennial Chimera," 1934, **113**, pp. 329 and 399.
(8) *Ibid.*, 1938, **121**, p. 286.
(9) *Ibid.*, 1938, **121**, p. 315.
(10) *Ibid.*, 1938, **121**, p. 467.
(11) *Journal I.E.E.*, "Axial Spin of a Magnet," 1936, **78**, p. 481.
(12) *Ibid.*, Discussion of item 11, 1936, **79**, p. 345.
(13) *Electrician*, "A Persistent Fallacy," 1936, **117**, pp. 191 and 493.
(14) *Ibid.*, "Magnets and Fields of Force," 1936, **117**, p. 625.
(15) *L'Éclairage Électrique*, 1900, **23**, p. 46.
(16) *Physical Review* (New York), 1912, **35**, p. 323.
(17) *Philosophical Magazine*, 1885, **19**, pp. 131 and 215.
(18) *Electrical Review*, "Torque of an Electrical Machine," 1934, **115**, p. 891.
(19) *Electrician*, "Torque on Armature and Poles," 1935, **114**, p. 763.
(20) *Ibid.*, 1935, **115**, pp. 416 and 471.
(21) *Ibid.*, "Some Magnetic Misconceptions," 1935, **115**, p. 601.
(22) *Electrician*, 1898, **40**, pp. 453 and 510.
(23) *Wireless Engineer*, "Stresses in Magnetic and Electric Fields," Dec., 1946, **23**, p. 319.
(24) *Ibid.*, "More Magnetic Fallacies," 1942, **19**, p. 239.
(25) *Nature*, "The Electromagnetic Mental Picture," 1942, **149**, p. 248.
(26) *Philosophical Transactions of the Royal Society*, A, 1905, **204**, p. 121.
(27) *Wireless Engineer*, "A Classic Experiment," 1939, **16**, p. 271.
(28) *Proceedings of the Royal Society*, A, 1913, **89**, p. 99.
(29) *Philosophical Magazine*, "A Paradox in Electrodynamics," 1945, **36**, p. 32.
(30) *Wireless Engineer*, "An Interesting Electrodynamical Problem," 1945, **22**, p. 469.
(31) *Transactions of the American I.E.E.*, 1923, **42**, p. 139.
(32) *Wireless Engineer*, "Size of an Electron," 1946, **23**, p. 33.
(33) *Ibid.*, "Problem of Two Electrons and Newton's Third Law," 1944, **21**, p. 105.